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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of macrophysical cloud parameters from MIPAS: algorithm description and preliminary validation" *by* J. Hurley et al.

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1 General Comments

The paper introduces an optimal estimation based method for retrieval of cloud properties from mid-infrared (midIR) limb sounder MIPAS. Examplary retrieval results from one month of data are presented and compared to ISCCP data. Cloud detection results of a one-day subset are furthermore compared against a different MIPAS cloud detection method, which uses so-called colour indices. In contrast to earlier published cloud retrieval methods from MIPAS, the described method allows for quasi-operational (i.e., applicable to a large set of measurement data not just case studies) retrievals. Being

able to process large data volumes is an important step towards comprehensive data analysis, exploiting MIPAS measurements with regards to clouds, providing an additional global cloud data set, and hence contributing to a more complete view on global cloud properties.

However, I have two major objections regarding (a) the validity of the method itself, and (b) the appropriateness of the preliminary validation against ISCCP data as presented here.

Method: The method is essentially based on the assumption of a cloud as a grey absorber, i.e., a non-scattering medium. Both ice and liquid water clouds, however, have significant scattering components at the wavelengths used for the retrieval (single scattering albedos roughly in the range 0.2-0.7 (Hess et al., 1998)). This issue and limitations of the model due to that are basically not discussed in the paper. The impact of scattering on the results and expected errors have not been estimated. Though concluding "The retrieval errors associated with application of this algorithm to this data can be used to determine a measure of confidence for how well the forward model represents realistic scattering clouds.", this has not been demonstrated in the paper nor has a reference been provided that could provide some verification.

Validation: For (preliminary) validation of the retrieved cloud parameters, ISCCP data has been used. ISCCP provides a fairly large and comprehensive set of data. It does not become clear, what exactly has been used in the validation/comparison. Besides, I doubt that the data from ISCCP that has been used here is appropriate for the intended purpose. MIPAS is particularly sensitive to high clouds (low clouds, according to my understanding, are hardly seen due to high opacity of lower atmospheric limb paths). Furthermore, the method applied here is set up in a way that highly emphasizes high clouds over lower clouds – a cloud is detected at (and just at) the highest cloudy limb measurement. That is, all lower clouds (e.g., multilayer clouds, or lower clouds at different horizontal position that might be in the field of view of lower tangent altitude measurements) are ignored/missed. Then, this "high cloud emphasizing" data set is compared to data, which might be the complete ISCCP data. We know, ISCCP data is more sensitive to thicker clouds, hence comparably emphasize lower clouds. Nevertheless, the shown data (Fig. 3) with mean tropical cloud top heights below 5km and southern polar cloud heights > 10km seems strange and, according to my opinion, needs some explanation. If ISCCP data shall be used, a more appropriate data set to compare to here should be ISCCP's high cloud data subset. However, in my opinion the best data to compare to in the case here might be CALIPSO data – although there might exist only few collocations, a statistical (one month average as anyway has been used here) comparison should still be possible.

2 Specific comments

Page 3878, abstract: *"three macrophysical cloud parameters (cloud top height, cloud top temperature and cloud extinction coefficient)"* – can cloud extinction really be subsumed under "macrophysical parameters"? To my understanding, it's an optical or radiative parameter, but doesn't fit in the micro-/macrophysical parameter frame. In this context I'd also question the later statement (page 3879, line 16) that cloud properties loosely fall in two categories, namely the micro- and the macrophysical ones.
Page 3879, first paragraph: I don't see, how/why the azimuth scanning and global coverage is relevant to mention – neither does apply to MIPAS.
Page 3880: Both your list of in-situ campaigns and climatologies seems out of date. While it is clear, that they can't possibly be complete, the lack of recent data is striking. On the campaign side TC4 is worth to mention, but even more on the climatology side I miss references to CloudSat, CALIPSO, MLS (e.g., Wu et al., 2008) and Odin/SMR data (e.g., Rydberg et al., 2009) sets, and the GEWEX cloud assessment (GEWEX cloud assessment; Stubenrauch et al., 2009) for example.
Page 3880, line 3: I don't see that satellite instruments are better or worse suited for measuring macrophysical properties than microphysical or radiative properties. In the end, it's always a (suite of) radiation related parameter(s) that is actually measured,

and some retrieval is needed to derive other properties.
Page 3880, line 13: *"for instance, microwave instruments often are not sensitive to ice cloud particles"* – I think, that highly depends on the type of ice cloud. Microwave instruments (and, I assume, the authors would subsume mm- and sub-mm-wave instrumentations under this term as well) are hardly sensitive to small particles as dominating in thin cirrus clouds, but do measure fairly strong signals from thicker cirrus (including cirrostratus), cumulonimbus and nimbostratus, etc. due to significant fractions of larger particles (>100 μm) contained in those ice (containing) clouds. Please consider that thin cirrus are not the only type of ice clouds.
Page 3880, line 15: *"It is thus important to choose to retrieve cloud properties appropriate to the satellite instrument's capabilities."* – the phrasing seems somewhat odd. Do you mean something like: one can only retrieve parameters that give a sufficient signal, which can be separated from other properties? I'd agree to that, adding that one has to carefully define the property one claims to measure (e.g., extinction or optical depth are wavelength dependent, i.e., MIPAS measured extinction is usually not the same as SCIAMACHY measured extinction of the same cloud), and the limitations of the measurement method (e.g., ice water path, effective size, etc. from different instruments largely differ since instruments are sensitive to different parts of the cloud).
Page 3880, line 26f: Could you give some reasons, why – apart from the sake of completeness – (very) thin clouds are of interest. Arguing somewhat provocatively, I'd say they neither contain significant amounts of ice nor modify the radiation field markedly (else they wouldn't be missed by most sensors).
Page 3880, line 28: *"limb-viewing has not been used for cloud measurements"* – what about MLS (both UARS- and EOS-borne) and Odin/SMR? According to my knowledge, EOS-MLS channels have even been designed (not only, but as well) with regard to ice cloud measurements.
Page 3881, line 3f: Is instrument sensitivity really depending on the detection/retrieval approach? Besides, "detection mechanism" sound strange, I'd rather use method or algorithm.

Page 3881, paragraph 2: I don't agree, that, although commonly done in practice, cloud parameter retrieval is necessarily dependent on (prior) cloud detection. According to my understanding, cloud parameters from Odin/SMR (Rydberg et al., 2009), for example, are retrieved from all data, without prior detection.
Page 3881, line 11ff: First, with all these parentheses the text is hard to read. So I am not completely sure, what this part actually refers to: *"which exploit the fact that clouds introduce increased radiance and extinction"*. While the statement is certainly true regarding extinction, it is not clearly true regarding radiance. This is only valid for certain viewing geometries and wavelength regions (downlooking IR and mm-wave instruments as well as sub-mm-wave limb sounders typically observe decreased signals). That is, the authors need to specify which part of the sentence refers to which instruments/method.
Page 3881, line 20: *"a decrease in certain specific constituent volume mixing ratios, such as ozone"* – this refers to a physical or a "virtual" (e.g., due to some cloud shading of the signal) VMR decrease? If it is physical, to what type of clouds does it apply? Please add a reference for that.
Page 3882, section 1.3: This seems to target a complete overview of cloud parameter retrieval attempts from MIPAS (using firstly, secondly, finally). Note, that Mendrok et al. (2007) have demonstrated retrieval of macro- (cloud height) and microphysical (effective particle size, ice water path) properties using the radiative transfer model SARTre.
Page 3882, line 25f: Is it relevant to mention *"under certain circumstances"*? If so, be a bit more specific, what circumstances that are.
Page 3883, line 6f: *"using an adaptation of standard retrieval theory"* – what is meant by "standard"? and what's the adaption that is made?
Page 3883, section 2.1: Why were the MWs all put in the rather narrow wavenumber interval $930-970\,\mathrm{cm}^{-1}$? Why is it 10 MWs (and not less or more)?
Page 3884, section 2.2: The definition of continuum radiances is crucial, since the retrieval is largely based on these values. Hence, a thorough explanation is necessary. In particular: The identification of points with minimum molecular contributions

is done for each measurement/limb scan? Or is this rather done once (when establishing the MWs)? How exactly is the "continuum radiance" defined? As climatological expected mean radiance over an MW? Or is this here rather plainly a definition of a variable "continuum radiance", which is a mean radiance over the previously selected microwindows? If so, the first paragraph of the section should rather go to the microwindows section.
Page 3885, line 3: Why/How has the retrieval method introduced here a "more physical basis"?
Page 3885, paragraph 2: Is R_c the measured, MW-mean radiance (see above: I had some difficulties understanding what R is and how it is derived), or is it modified by some simulated clear-sky radiance? Taken the first applies – though in clear-sky case R_c is low, it might not be negligible, in particular for lower tangent altitudes (By the way, what tangent altitude range is taken into account in the retrieval? And how far down does MIPAS measure? That should be mentioned in the text somewhere.). Hence, low altitude CEF will not even in clear-sky case become 0. Furthermore, at the wavelengths used here, is it always sure that (a) clouds can not cause higher signals than B_c, e.g. due to scattering, and (b) that clouds increase (and not decrease) the measured signal compared to clear-sky case? How is cloud scattering expected to impact the CEF estimate?
Page 3885, paragraph 5: What is R_ν, and what is it used for (it never appears before or after in the paper)? Is this computed from CEF, or CEF computed using R_ν? What is meant by "lower" and "upper fraction" α? What is "spectrally varying" about R_ν and how does that fit with using spectrally averaged variable B_c? How is τ_ν defined – spectrally averaged as well, and over which region? How well is the usage of climatological transmittance justified? what deviations to actual transmittance at observation time are expected? How do these deviation impact the results? How well is the assumption justified, that Eq. (2) hold for other in-FOV distributions of the cloud?
Page 3886, line 7ff: More information on constraining B_c, the setup of the optimal estimation scheme that is already used here, and errors of estimated CEF would be

helpful.
Page 3886, line 10: Does that mean, only the uppermost cloud layer is detected, and all lower tangent altitude measurements are "thrown away"?
Page 3887, line 7: Extinction coefficient and cloud extinction are spectrally dependent parameters. Hence it is necessary at some point to state that extinction (coefficient) around 950 $\mathrm{cm}^{-1}$is retrieved.
Page 3887, line 12: Is R_c here the same continuum radiance R_c as before, though R_u and R_l are also continuum radiances? To my feeling, there is some confusion of actual variables and concepts/parameter definitions. Are the three continuum radiances actual measured values, or have they been modified/computed before?
Page 3887, line 17f: *"α depends on the spectral structure whereas R_c is derived from the spectrally flat regions"* – hasn't α been derived from R_c? Where does the spectral structure come in? I do not read that from Sect. 2.3.
Page 3888, line 12f: *"This acts as a constraint on the CTH and CEX values, as described in Sect. 2.3"* – as before, I do not read that in Sect. 2.3.
Page 3888, line 28: *"with a large uncertainty $\sigma_{\mu a} = \pm 0.5$"* – is that really a large (enough) uncertainty for clouds?
Page 3889, line 12f: *"it is assumed that the Planck function [...] varies linearly"* – how well holds this assumption over the necessary height scale? how large are the expected deviations? How much could that affect the retrieval results?
Page 3891, line 15ff: What does this (linearly varying cloud radiance vs. above cloud step function) mean in practice? Do the a_j vary depending on z_c?
Page 3896, line 9ff: How can you by qualitative comparison conclude MIPAS sees more thin cloud than ISCCP (a clear quantitative result)?
Page 3896, line 18ff: *"Generally, the retrieval is able to estimate the cloud top height within 50 m, cloud top temperature within 0.5 K, and extinction to within 15%."* – That's what the covariance matrices give, which mentioned by the authors themselves are an optimistic assumption. I am not convinced, that the error inflation reproduces a proper error level. Error inflation is based on between microwindow scatter. However, when

the errors are correlated (as can be expected since assumptions for the different microwindows are very similar), systematic errors are missed.
Page 3897, line 7ff: What is the target of CI, and of the operational threshold in particular? Cloud-conservative or clear-conservative cloud "masking""? Please add a reference for operational threshold. Is this indeed altitude independent, since both clear-sky CI and cloud effects depend on tangent altitude?
Page 3898, line 2f: *"confirms [...] can be successfully retrieved"* – since I don't think the validation is done properly here, I can only judge that something reasonable is retrieved. Whether it is indeed good, valid, or "successful" is not possible to decide.
Page 3898, line 5f: *"has been tested and found reliable on real MIPAS data"* – reliable in terms of what? See above - proper validation is missing, so it's impossible to judge.
Page 3901, caption: *"ordered in terms of priority of selection"* – if mentioning that there is some priority selection, some more information how/why this selection is done would be nice.
Page 3902 (contentwise Sect. 2.1 and 2.2, I guess): Obviously, selected MWs not only include continuum regions, but also fairly strong CO_2 lines. What is the benefit of that? How are they included in R_c and R_ν? Is spectral data in the MW used in average form or on MIPAS wavenumber grid points?
Page 3903: I suggest to select plot symbols in a more "linearly" varying way, e.g., use filled circles for type1 retrievals, open circles for type2, to make type1 that is the most complete retrieval easier catch the eye. Nevertheless, it is rather difficult to recognize the exact color of the open symbols and compare them to the filled symbols.
Page 3904: At which is wavelength ISCCP optical depth defined?
Page 3905: Is there an explanation for the 16 km peak of CEX errors?

3 Technical corrections

Page 3879, line 11: add comma after “however”
Page 3881, line 25ff: Are the parantheses correct? The item starts with opening a parantheses, which seems odd.
Page 3885, line 16: add “and” after “radiance”
Page 3885, line 25: “τ_ν the same” → “τ_ν is the same”, “transmittance used in Sect.” → “transmittance introduced in Sect.”(?)
Page 3886, line 7: “a priori” → “*a priori*”
Page 3893, line 1: “is not great” → “is small” (Maybe also give an order of magnitude estimate to underline that statement.)
Page 3894, line 16: Explain “sweep” (first occurence here).
Page 3896, line 16ff: Does $\mathbf{S}_x$ here (and the retrieval errors shown in Fig. 4) refer to $\mathbf{S}_x$ (single microwindow covariance), $\hat{S}_x$ (microwindow averaged covariance), or $\hat{S}'_x$ (error inflated microwindow averaged covariance)?
Page 3897, line 1: “In this section, ...” – a verb seems missing.
Page 3897, line 2ff: This sentence needs re-wording. It’s not clear (to me), whether you take the spectra above or below cloud tops.
Page 3897, line 25: “which is so frequently missed” – what’s the function of “so”?
Page 3900, line 12: the Thomas et al. reference is out of alphabetical order.

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